indistinguishable morphologically from *T. rhodesiense* should be disseminated amongst horses by coitus is of considerable interest. We are at present conducting experiments with a view to ascertaining whether these three strains, all of them obtained from horses suffering from "mal de coït," are still capable, after numerous passages through laboratory animals extending over many years, of being transmitted in equines by coitus. For the present, we propose for this *rhodesiense*-like trypanosome the name *T. equi.*

DESCRIPTION OF PLATE.

Drawn with Abbé camera lucida, using 2 mm. apochromatic objective and No. 12 compensating ocular (Zeiss). Magnification 2000 diameters.

Figs. 1- 6.—Strain A (Berlin Strain).
Figs. 7-10.—Strain B (Frankfurt Strain).
Figs. 11-14.—Strain C (East Prussian Strain).

Studies in the Heat-production Associated with Muscular Work. (Preliminary Communication: Section A.—Methods; Section B.—Results.)

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Section A.—Method.

The calorimeter with which the included data have been obtained was built upon the plan described by Benedict,* omitting, however, such parts as were essential rather to a study of the respiratory gases than to measurements of heat-production. The general principles of its construction are well known, exceedingly ingenious, were developed by Atwater and Benedict, and are briefly as follows: The body of the calorimeter is of sheet copper built upon an external wooden framework, on which again is built externally an outer zinc box enclosing, but nowhere in contact with, the calorimeter box proper. Between the two metal boxes, sets of thermocouples arranged in groups are utilised to discover any differences of temperature likely to lead to a radiation of heat from one box to the other across the intervening air space partially occupied by the wooden framework. In the walls of a still

* 'A Respiration Calorimeter, etc.,' published by the Carnegie Institution of Washington, 1905.

more external wooden shell are placed means by which heat may be added to or subtracted from the zinc box in a graduated fashion so as to annul any such observed differences in temperature. Thus the zinc box is kept in each of its several zones, each zone corresponding to a group of thermocouples, at the same temperature as the copper box, and the thermal insulation of the calorimeter is thus insured.

The subject of the experiment enters the calorimeter by a window space left in the walls of this nest of boxes, and is then sealed in by glass and wax. The heat produced in his body, as well as the heat into which all his mechanical work is finally converted, raises the temperature (1) of an insulated radiator system through which a steady stream of water is maintained; (2) of the calorimeter box; (3) causes some evaporation of water from his respiratory passages and skin, and (4) tends to raise his own temperature. Each one of these four stores of heat is observed in suitable ways, and the summed account of their alterations provides a measure of the heat-production of, or total transformation of energy in, the subject.

Now, although the main principles of construction are the same as those of the original calorimeter in the Middletown University, I have altered many small details, in part for the sake of economy, in part for convenience, and in some small degree with a view to improvement. Most of these I hope to describe briefly in a more extended communication; to one alone, as somewhat modifying the usage of the instrument, I refer at present. This modification consists in the introduction of a source of heat other than the man, in some cases adding substantially to the total heat-production. The main reason for this change in procedure was a desire to follow the progress of changes in the heat-production of the subject more closely than was possible with the original arrangement.

Whilst endeavouring to follow the events of shorter periods of time, *e.g.*, complete observations every 5 minutes, it was soon found that of the four stores into which the heat produced by the subject was delivered, one alone, the temperature of the calorimeter, had a sufficiently elastic capacity to follow abrupt changes. Associated, however, with the possession of this advantage was the failing that, thus abruptly changed, it tended to form a similarly abruptly changing site of heat leakage, unless rapidly checked by adequate and almost simultaneous adjustments in the temperature of its environment, that is of the enclosing zinc box. On this account it is necessary that modifications in heat-production must be kept well within the limits of adjustment of this process, which by the way may conveniently be termed the balancing of the calorimeter. It is only when such limits are not transgressed that any reliance can be placed upon the value of such

corrections for changes in the temperature of the calorimeter as may be ascertained by appropriate calibration experiments. I have therefore placed five incandescent lamps within the calorimeter, reading the power absorbed by them by means of a wattmeter, and changing this value by means of suitable resistances. In such a way it is possible to compensate alterations in the heat-production of the subject, and to dispense with the necessity for any but the smallest adjustments in the balance of the calorimeter. It is clear that such a process of compensation may be managed with greater abruptness and precision than complicated modifications in the balance, using the constantly observed temperature of the calorimeter as an index of success or failure.

In addition to the subject and these lamps two other sources of heat have been present within the calorimeter during these experiments. Thus, in the first place, there is the power-absorption of electromagnets forming part of an electrical brake applied to the cycle-ergometer upon which the subject performs definite amounts of mechanical work, and this is followed by a voltmeter and ammeter arranged in this circuit. Two such electromagnets placed in separate positions each environ a part of the path traversed by the periphery of a copper disc by which the hind wheel of the cycle has been replaced. Benedict has recently described the calibration of such an instrument provided with a single electromagnet and with a thicker copper disc. He is fortunate in possessing in this machine an instrument in which, at a certain useful revolution rate, small changes of speed occasion directly proportionate variations in the amount of mechanical work performed. This useful property has been found by Benedict and Cady to be attributable to the reaction of the eddy-currents in the copper disc upon the value of the electromagnet exciting their presence, and is dependent upon the dimensions of the poles of the magnet and upon the thickness of the copper disc and its resistance. I am not so fortunate, and the mechanical work performed on my cycle varies more rapidly, rising with the square of the revolution rate. In this case it should be noted that the maintenance of a uniform speed is of greater importance.

To return to the sources of heat within the calorimeter, I have in the second place to mention the power-absorption due to a fairly powerful fan placed within the calorimeter and maintaining there the conditions of a moderate breeze. Here again a voltmeter and ammeter are used to assess the heat-production due to this source. This value added to that due to the brake and to that due to the incandescent lamps provide a "subtraction" which has to be taken from the total heat measured at any time before the heat-production of the subject is known.

Now I am aware of the fact that each of these additional sources of heat is also an additional source of error, subtracting to some degree from the theoretical precision of the instrument and diminishing its value as gauged by the measurement of any standard source of heat. In the practical usage of the instrument as applied to varying sources of heat they have a great value, which, in my opinion, fully justifies their insertion. Of the value of the lamps I have already spoken. The fan is used to keep the air inside the calorimeter in a state of satisfactory admixture, so that samples withdrawn from it in the general air-current are given a value in reference to the general air within the calorimeter of a simple and easily determined kind, and may thus be used to estimate changes in the value of this air. In these particular experiments this point is only of importance in dealing with the storage of aqueous vapour within the calorimeter.

Air is made to enter the calorimeter through a tube guarded by thermo-couples, a connected set being placed in the exit tube. Thus any difference between the temperature of the entering and leaving air is known. Means for warming the entering air are provided, but I have not so far made any arrangements for cooling it when necessary. The adjustment of these temperatures has not then always been as good as might be, but they are also observed by means of mercurial thermometers and any observed difference allowed for.

Air is sucked out of the calorimeter by a rotary pump at a measured rate, varying in these experiments from 300 to 450 cubic feet per hour. In the tube forming the path of the leaving air, and in the similar tube for the entering air, a dilatation is provided in the form of an interpolated glass box. In these glass boxes "wet and dry bulb" thermometers are placed, the stems outside for observation, so that an estimate may be formed of the excess aqueous vapour derived from the calorimeter. In my earlier experiments the water-vapour was weighed after absorption by sulphuric acid, or rather a fraction obtained from a section of the air-path was thus treated, but this plan was abandoned after comparative trial, mainly as not lending itself so well to the observation of five-minute periods of heat-production. Here perhaps the interests of absolute precision have apparently suffered from the desire to study the events of shorter periods of time.

Much more important than this path of heat escape with the aqueous vapour of the air current is the main path which issues along the stream of water from the internal radiator system. This radiator system, originally distributed along the line of junction of the walls and roof of the calorimeter, I have extended so as to be co-extensive with the roof of the calorimeter, beneath which it is suspended in an insulated fashion. The entering water is

driven to it from a constant-pressure supply through coils of tubing situated in ice-filled tanks, and arrives at a temperature sometimes less, sometimes greater, than 5° C. It passes out into a balance, such as that described by Benedict. This balance is duplicate, and automatic arrangements provide that the filling of the collecting pan suspended from one beam shall immediately be followed by admission of the stream of water into the similar collecting pan suspended from the second beam. The change over is made known by the ringing of an electric bell, and the weight then ascertained by the observer thus warned. Thus, this is the only set of readings which is not arranged in five-minute periods, and average rates of flow have to be accepted for each of the included five-minute periods. The intervals of such readings have varied from 12 to 20 minutes.

The remaining readings are taken by the observer every time warning is given by a bell attached to a five-minutes clock, in order somewhat as follows :—

(*a*) The wet and dry bulbs in the entering and leaving air.

(*b*) The thermometers in the entering and leaving water.

(*c*) The resistance of some 570 ohms of iron wire arranged on a series of coils within the calorimeter, by means of which its temperature is assessed.

(*d*) The number of revolutions of the cycle as read upon a cyclometer driven by an electromagnet from contacts on the cycle.

(*e*) The watts due to the lamps (wattmeter).

(*f*) Voltmeters and ammeters connected with the circuits of the fan and of the cycle-brake.

(*g*) The surface temperature; and

(*h*) the rectal temperature of the subject, as evidenced by galvanometric deflections due to suitable thermocouples.

(*i*) The temperature of an incubator in which are placed the "constant temperature junctions" of these thermocouples.

The data collected in this way have been dealt with on a uniform plan throughout the whole series of experiments here included. The only details of this plan which perhaps should be dealt with briefly here are (1) the means of applying the correction for the temperature of the calorimeter, and (2) for estimating the storage value of the air space in the calorimeter.

By suitable calibration experiments it has been found that an alteration of 0·01 ohm in the resistance of the calorimeter "thermometer" is equivalent to an addition, or subtraction, of 1/6 kilo-calorie (or kalorie, as written elsewhere in this paper). Now, in making up my accounts, I have expressed all the results in rates of change per hour, so that a change of this value observed in a five-minutes period (1/6 kalorie gained or lost in five minutes) is expressed as a change of 2 kalories per hour in the rate of heat-

production. Measurements of the resistance of a sample of the iron wire used in the construction of the calorimeter "thermometer" were carefully undertaken by Dr. Chapman, to whom, in the earlier stages of the construction and calibration of the calorimeter, I am indebted for a considerable amount of help. It was then found that a change of 1° C. in the temperature of this wire was accompanied by a change in the resistance such as, applied to the amount of wire in the calorimeter, would correspond with a change of 2·54 ohms in resistance; and this, seeing that 0·01 ohm involves a change of 1/6 kalorie, would involve a change of 42·3 kalories. Thus the thermal "water equivalent" of the calorimeter would seem to have the high value of 42·3 litres.

As to the storage space within the calorimeter, that is a figure which, for the present, is of much less importance, since the data given are taken from a time of work-performance when such storage is practically at an end. The storage space is, as we have found, less than the total internal air space. This fact has been ascertained by my assistant, Dr. Duffield, in calibration experiments, in which carbonic acid was delivered into the calorimeter at a definite rate, and comparison was made between this rate of entrance and its observed rate of departure in the air current. These experiments are in progress and will be described later by Dr. Duffield. In the meantime, for the purposes of this communication, I have taken the storage space as less than the total space by a quantity equivalent to that of the air entering over a single five-minute period. It is probable that later I may have to increase this subtraction, but the relative unimportance of the point may be judged from the values given below. These are the average figures provided by the set of experiments described as "Group D" in the next section, and from them it is possible to see how small the figures for the storage of aqueous vapour are, and how little their modification will mean when the "storage space" is better known.

Data from Group D (see p. 109).

		Kals.
(I)	Heat carried away by the water-stream	303
(II)	Heat carried away with the aqueous vapour of the air-stream	28
(III)	Heat stored in the water-vapour of the calorimeter	2
(IV)	Heat stored in the walls of the calorimeter (calorimeter temperature)	10
(V)	Allowed for difference between the temperatures of the entering and leaving air	1
(VI)	Allowed for change in the rectal temperature of the subject	2
	Heat production	346

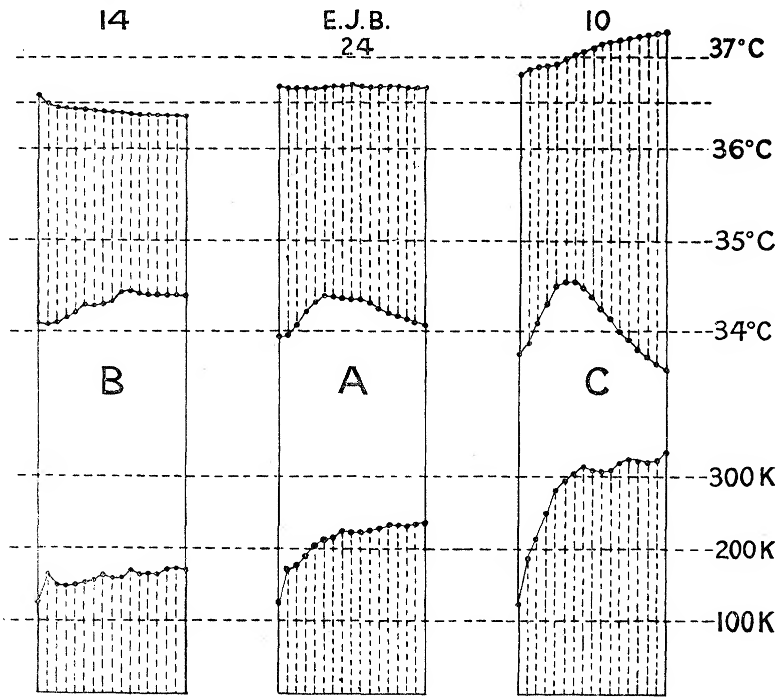
The staff at work upon the calorimeter in most of my earlier experiments consisted of two men; later, and in the whole of the experiments to which reference is made here, of three men. I must mention in the first place my laboratory assistant, Mr. A. Wallis, who has been a most valuable aid in the construction of the calorimeter and in the design and construction of accessory apparatus. In these experiments he has been responsible throughout for the balance of the calorimeter surface, and has kept records of the currents due to the various sets of thermocouples of very considerable importance to a discussion of the data. In the second place the observer, assisted by a note-taker, who recorded the observations as they were made and immediately dictated. At the present time this staff is increased by Dr. Duffield, who is now responsible for the gas-analysis side of the investigation. If I may forestall one of his measurements here, it is to record the fact that the air space of the calorimeter is 176 cubic feet. To obtain this measurement carbonic acid gas was delivered into the calorimeter, the enclosed atmosphere then thoroughly mixed by the fan, and its percentage content of CO_2 obtained from a sample. The rotary pump was then started, and the total amount of the gas ascertained as it was gradually withdrawn with the air current. The figure agrees very closely with that obtained by measurements of the average length, width, and depth of the calorimeter, but is of value because of uncertainty due to irregularities in the walls and because of a subtraction made necessary by the presence of the radiator system, etc.

A most important part of the method has been the securing of financial assistance, and in this connection I owe much to the British Association.

Section B.—Results.

The accompanying figure may serve to illustrate my need for considering the results of these experiments under two headings: (1) the events of the first hour of cycling; (2) the events of the second hour. Fig. A represents an average of data obtained in 24 separate experiments upon E. J. Briscoe at various known revolution rates of the cycle, and with various known values of the electrical brake, covering the whole range of work-performance upon the cycle which he could maintain uniformly for the required length of time. The lowermost curve in the figure is that of the heat-production; the middle curve gives his surface temperature; the uppermost is the rectal temperature. Fig. B is the average curve of such of these experiments—14 in number—as are below the general average of the full series. Fig. C is the average curve of the 10 experiments which lie above the general average of heat-production.

In each figure the different curves of each set (A, B, C) have the same meaning. Thus the lowermost always represents the heat-production, etc. Their examination will show that the experiments have been balanced so as to yield a general average such that in it the rectal temperature is practically uniform throughout, and is represented by what is almost a straight line (fig. A). It follows, therefore, that the underlying curve of heat-



production is practically devoid of any corrections due to alterations in the temperature of the subject. Nor will any modification in my system for dealing with such corrections, nor any new and quite different system, affect this fact, so long as such systems are based upon observations of the rectal temperature. Now the importance of this point is this, that the curve of heat-production is seen gradually rising towards a maximum. Thus although mechanical work was being performed at a uniform rate throughout each experiment, and, therefore, at an average uniform rate throughout the whole series of experiments, it would appear that the "heat-production" or total amount of energy transformed which was associated with its performance was not uniform, but that it increased during the progress of work performance. There is, however, no confirmative evidence as to any state of affairs like this obtainable from investigations of the primary

chemical changes underlying this transformation of energy. This is true of such experiments in general, and in this particular case has been found to be true by Dr. Duffield, investigating the carbonic output in similar experiments in the calorimeter. There is little doubt that the curve of transformation of chemical energy has a much greater parallelism to that of work performance, and would, therefore, in this case be represented by a straight line. It follows then that my experimental curve of "heat-production" has only a secondary relation to the real curve of transformation of energy, and that I am losing sight of some part of the energy transformed, large at first, but subsequently diminishing. I find myself therefore obliged in dealing with the events of the first hour to consider the following possibilities :—

A. That a storage of heat in the disc of the cycle, representing some large fraction of the mechanical work done, is of great importance, and has been neglected.

B. That there is a storage of heat within the subject, as, for instance, within his musculature, to which the rectal temperature is no adequate index.

C. That some of the energy transformed in the oxidation processes accompanying the performance of work is stored in the body in some form other than heat.

Now, the first of these possibilities, dealing with the rising temperature of the copper disc of the cycle, is certainly a source of some error. It cannot, however, be considered as a source of errors of the magnitude present in this case, since at the most it could hardly account for more heat than is due to the conversion of the mechanical work performed, and this value is no more than a small fraction of the missing quantity. Obviously again, the third, and extremely interesting, possibility is best left on one side until the second one is adequately considered, namely, that under working conditions the rectal temperature fails to represent the real average temperature of the body. It has been noted that, during conditions of rest, the rectal temperature may be taken as an index to variations in the real average temperature, even if it is not on absolutely the same level as that average temperature, since the variations in the two are probably similar. Thus, it has been observed that temperature observations taken under such conditions at several different sites (*e.g.* skin and rectum) give parallel curves. In this case there is no such parallelism, as may be seen from the curves of rectal (uppermost) and surface (middle) given in the figures.

It is, indeed, probably the case that the rectal temperature represents no more than the mean temperature of the mixed blood sent from the heart to one part of the general circulatory system, a particular part in which, owing to an absence of much local performance of work, this mean temperature is

only slightly changed, so that the mean temperature of the gut is much the same as that of the arterial blood entering it or of the venous blood leaving it. The mixed blood that is sent from the heart, however, may simply, and for the moment, be thought of as blood from the cooling district of the skin plus blood from the warm district of the musculature, and, whereas the blood from the skin may fairly represent the temperature of the mass that it traverses there, it is not at once obvious that the blood returning from the great bulk of the skeletal musculature, although of a quantity sufficient to cope with the demands of a greatly increased oxygen-transport, should at the same time be so increased as to keep the musculature at a temperature only slightly greater than its own. The rectal temperature may conceivably fail under such circumstances as an index of the average temperature because the muscles are not represented in their proper proportionate value. However, these are the difficulties of dealing with the events of the first hour of cycling, and I propose to leave them on one side for the present, until in a more extended form of publication I can deal with the details of corrections for the temperature of the calorimeter and of the other measurements which add to form this picture. We shall therefore pass at once to a consideration of the events of the second hour.

The Second Hour of Cycling.

In each of the experiments represented by the data given subsequently the subject entered the calorimeter, and having been sealed in, remained seated until the balance of the surface of the calorimeter was well under control. He was then signalled to seat himself on the cycle, and at a second signal started to cycle at a fixed rate of 60 revolutions per minute. Outside the calorimeter window an incandescent lamp was made to glow 60 times a minute by the establishment of short-lasting contacts, one per second, by a seconds-clock, and it was the cyclist's duty to keep pace. His performance was watched by the cyclometer, and he was informed if the variations in his pace were noticeable. As a general rule, however, the rate was excellently maintained, and I owe many thanks to my long-suffering subjects, who pedalled away in this monotonous fashion in each case for a period of two hours.

The values of the electrical brake were originally arranged so as to separate the experiments into two groups. In one group the work done upon the cycle was to be maintained at a value of 13 kalories per hour; in the second group at a greater value, 43 kalories. These values were obtainable by the maintenance of the 60-per-minute revolution rate in each case and by sending in the one case a current of 1 ampère and in the other of

2·1 ampères through the electromagnets of the cycle, as was known from the results of calibration experiments most kindly performed for me in the Electrical Engineering Department of the University by Mr. Bissett, under the supervision of Mr. Crapper, to both of whom I am greatly indebted.

When a considerable number of such experiments had been performed with results all placed within certain limits of the scale of possible heat-production I was suddenly introduced to two simultaneous phenomena, (*a*) the ammeter connected with the circuit of the cycle brake, although mainly preserving an appearance of carrying the required currents, yet showed sudden intermissions during which the current evidently fell in value for short periods of time that seemed to be associated with some regular phase in the revolution of the cycle, and (*b*) that my experimental results now began to occupy new regions in the scale of possible heat-production. An examination of the electromagnets of the cycle-brake revealed a wire insecurely held within a soldered joint in such a fashion that the total resistance of the joint must have been very variable.

This fact is my quite sufficient excuse for omitting this particular group of experimental results. It also involved the complete rewinding of the electromagnet, since the fractured wire was buried by other coils, and therefore very inaccessible. I was thus involved in further consequences since I was obliged to recalibrate the cycle-brake within the laboratory. Two sets of calibration experiments were performed, (1) with 1 ampère, and (2) with 2·1 ampères in the altered brake. I now, however, found myself in each of these cases possessed of a more powerful brake than before, and rather than spend further time in discovering the particular values of current which would give precisely the same brake-power as formerly I proceeded to perform two new groups of experiments, in one group 19 kalories being the measure of the mechanical work performed per hour, and in the other case 56 kalories. I have therefore now to deal with four groups of experiments, and from the results it might perhaps appear that this is an advantage rather than a misfortune.

Before detailing these results I should make some mention of the fact that in addition to omitting a certain group of experiments previously referred to, there are some others which I have deliberately omitted from my list. In the first place, one of my subjects, a laboratory boy, Armstrong, promised to be of great advantage because of his smaller weight of 44 kgrm., thus occupying a different region of the scale of weights than most of my other subjects. He had, however, no experience of cycling, and was thus, in my opinion, most handicapped in the experiments with only a slight resistance in the brake. Given plenty of work to do on such a treadmill the best way of

performing it is soon discovered and adhered to when found, but with the scanty work of the light brake many needless additional movements may be performed without much personal inconvenience. On this ground I have omitted his earlier experiments, which were of this kind. Now on similar grounds, namely, that excessive movements were seen and commented upon at the time, I have omitted two experiments with the lightest brake performed by two other individuals. In both cases the appearances were so marked that these subjects were requested to provide me with another opportunity, and were kind enough to grant this. One of these repetitions, however, fell unfortunately in the period of ambiguity of the brake, the other coincides with the time of the altered brake and is recorded there.

With these exceptions all the experiments of this type performed since October, 1912, are summarised in the data given. Those experiments which are quoted from an earlier date than this are selections satisfying the conditions adhered to this year during this whole series of experiments, and are taken from a time when I was experimenting with various revolution rates and brakes as well as with modifications in handling the calorimeter. As a matter of fact this process of selection has involved the exclusion of only two experiments from amongst those which might have been available; one because the cycling was not continued sufficiently into the second hour and the other because of a quite unusual difference between the temperature of the entering and leaving air and between the temperature of the laboratory and that of the calorimeter. In no case except that of Armstrong would the inclusion of these experiments have altered the general character of the results obtained.

Group A.—Experiments in which a maintained rate of 60 revolutions per minute would have involved the performance of mechanical work on the cycle at the rate of 13 kalories per hour (approximately 0·02 horse-power).

	Date.	Weight.*	Revolutions per minute.	Name.	Heat production, in kalories per hour.
	1912.	kgrm.			
I	Nov. 13	54·6	59·4	Bennet	160
II	„ 18	58·8	60·1	Ward	181
III	„ 21	66·7	60·1	Sharrard	209
IV	May 22	55·7	60·7	Briscoe	169
V	„ 25	61·8	59·7	Chapman	184
VI	„ 30	62·0	59·8	Duftey	186

Average heat production 182 kalories per hour.

Maximal aberrations from this average $\begin{cases} +12\cdot4 \text{ per cent.} \\ -13\cdot9 \quad „ \end{cases}$

* All weights given were taken with the subjects stripped.

Group B.—Experiments in which a maintained rate of 60 revolutions per minute would have involved the performance of mechanical work on the cycle at the rate of 19 kalories per hour (approximately 0·03 horse-power).

	Date.	Weight.	Revolutions per minute.	Name.	Heat production, in kalories per hour.
	1913.	kgrm.			
	Jan. 28	54·6	60	Bennet	205
	„ 29	54·6	60	„	190
	Feb. 4	54·6	60	„	202
	„ 3	54·6	60	„	182
	„ 5	54·6	60	„	188
I	—	54·6	60	Average of Bennet	193
II	Feb. 17	62·1	59·8	Kemp	218
III	March 3	50·3	60	Gamm	197
IV	„ 4	60·5	60	Rae	212
V	„ 5	43·7	60	Armstrong	177

Average of I, II, III, IV, V, 199 kalories per hour.

Maximal aberrations $\begin{cases} +\ 9·5 \text{ per cent.} \\ -10·0 \quad „ \end{cases}$

Group C.—Experiments in which a maintained rate of 60 revolutions per minute would have involved the performance of mechanical work on the cycle at the rate of 43 kalories per hour (approximately 0·07 horse-power).

	Date.	Weight.	Revolutions per minute.	Name.	Heat production, in kalories per hour.
	1912.	kgrm.			
	Nov. 7	54·6	59·8	Bennet	278
	„ 11	54·6	59·6	„	280
	Dec. 5	54·6	60·0	„	283
I	—	54·6	59·8	Average of Bennet	280
II	Nov. 14	58·8	60·0	Ward	298
III	„ 15	66·7	60·0	Sharrard	324
IV	„ 25	59·9	60·1	Turnbull	299
V	Dec. 2	60·5	60·0	Rae	317
VI	„ 18	43·7	60·0	Armstrong	279
VII	May 20	55·7	60·6	Briscoe	285

Average of I, II, III, IV, V, VI, VII, 297 kalories per hour.

Maximal aberrations $\begin{cases} +9·1 \text{ per cent.} \\ -6·1 \quad „ \end{cases}$

Group D.—Experiments in which a maintained rate of 60 revolutions per minute would have involved the performance of mechanical work on the cycle at the rate of 56 kalories per hour (approximately 0·09 horse-power).

	Date.	Weight.	Revolutions per minute.	Name.	Heat production, in kalories per hour.
	1913.	kgrm.			
	Feb. 18	62·1	60·4	Kemp	354
	„ 26	62·1	60·2	„	345
I	—	62·1	60·3	Average of Kemp	350
	Jan. 27	54·6	59·7	Bennet	338
	„ 30	54·6	60·0	„	332
	„ 31	54·6	60·1	„	336
	Feb. 13	54·6	59·0	„	333
	„ 19	54·6	60·0	„	338
II	—	54·6	59·8	Average of Bennet	335
III	Feb. 20	60·5	60·4	Rae	347
IV	„ 21	60·4	60·5	Hill	345
V	„ 24	68·3	60·6	Sharrard	352
	„ 25	43·7	60·4	Armstrong	340
	„ 28	43·7	60·3	„	352
VI	—	43·7	60·4	Average of Armstrong	346

Average of I, II, III, IV, V, VI, heat-production of 346 kalories per hour.

Maximal aberrations $\begin{cases} +1·7 \text{ per cent.} \\ -3·2 \quad „ \end{cases}$

Now, it is always of interest to study the relationship between such figures, and significant powers of the subjects' weights: that is to say, such powers as W the weight; or $W^{2/3}$, possessing some reference to the subject's extent of surface; or $W^{1/3}$, in which there always lies the possibility that it is $W/W^{2/3}$, or the subject's weight divided by his surface. That there is a very real interest in such a quest is well known in the special case of heat-production during rest, which bears a fairly close numerical relationship to the extent of the surface when other conditions remain the same. I have therefore divided the figures in these different groups by $W^{1/3}$, $W^{2/3}$, W, $W^{4/3}$, and $W^{5/3}$, thus obtaining, in each case, a set of figures with a certain average value, and give, in the table on p. 110, the maximal aberrations observed from this average value.

Maximal Aberrations, given as a percentage of the average result of dividing by various simple functions of W.

	$W^{1/3}$.	$W^{2/3}$.	W.	$W^{4/3}$.	$W^{5/3}$.
Group A	+ 9·5 −10·2	+ 7·6 − 5·8	± 3·3	± 2·5	+ 5·1 − 4·5
Group B	+ 4·4 − 2·7	± 3·6	+ 9·8 − 5·6	+16·8 −10·0	+20·7 −17·3
Group C	+ 5·5 − 4·9	+11·4 − 3·5	+22·0 − 7·0	+32·0 −12·4	+43·0 −18·4
Group D	+11·5 − 9·0	+19·8 − 9·0	+30·6 −14·9	+39·0 −19·7	+55·0 −25·0

Now, compare with the figures in this table the maximal aberrations from the average experimental result as given in the preceding tables.

Maximal Aberrations from the Mean Experimental Result.

Group A	12·4	and	−13·9
Group B	9·5	„	−10
Group C	9·1	„	− 6
Group D	1·7	„	− 3·2

It will be seen at once that the experiments of Group D gain nothing by any new process of numerical treatment. The experimental results are almost constant notwithstanding considerable differences in the weights, and therefore in various powers of the weights, of the subjects; and, indeed, if the observations were sufficient in number, it would be justifiable to write the general summary of the results obtained at this level of mechanical-work performance as

$$\text{Heat-production} = K_d.$$

If, on the other hand, we go to the other extreme and examine the results in Group A the original errors are as large as 13 per cent. of the mean experimental result, and it is seen that division of these results by almost any one of these functions of the weight brings them to a more common level, but that this is best done by dividing them by $W^{4/3}$, when the aberrations are only 2·5 per cent. from the average. Here, we might say that, at this much lower level of mechanical-work performance,

$$\text{Heat-production} = K_a W^{4/3}.$$

Treating the other groups in the same way we get the following summary of the experimental results :—

$$\begin{aligned} &\text{Group A.—Heat-production} = K_a W^{4/3} \\ &\text{Group B.—} \quad\quad ,, \quad\quad = K_b W^{2/3} \\ &\text{Group C.—} \quad\quad ,, \quad\quad = K_c W^{1/3} \\ &\text{Group D.—} \quad\quad ,, \quad\quad = K_d \end{aligned}$$

Clearly these results demonstrate the decreasing influence of the weight upon the heat-production as increasing values are given to the performance of mechanical work. To put the facts in simple terms, without attempting any further analysis, the weight becomes less and less of a handicap as the rate of work is increased, until at the final level reached in these experiments the burden of the day is the same for all. This does not say that it may be borne equally well by all. Small bodies embroiled in an equal heat-production are obviously at a disadvantage, since, although their surfaces and thus their means of heat loss are relatively large in proportion to their weights, yet they are actually smaller than the larger surfaces surrounding the greater masses. Thus higher temperatures might be thought of as impending in their case.

It will be of interest to continue these experiments, and that, too, at higher levels of work-performance. It would, for example, be well to determine whether this was the end or not of this process in the removal of the handicap of the weight. It would be remarkable if at some higher level of work-performance the weight should be developed into a positive advantage, and the relationship be capable of expression as follows:—

$$\text{Heat-production} = KW^{-n}.$$

Leaving this, however, for the present alone, it is legitimate to inquire into the possible causation of these results as they stand. Thus, when dealing with the nature of the process responsible for the appearance of the weight in these results, it will be well at once to focus attention on this query: Is any importance to be assigned to the observation at the lowest level of work-performance, in which the weight made its appearance in the greater dignity of the form of $W^{4/3}$? For, if not, it would be simple to consider that all of these expressions are variations from the well known expression for the heat-production of rest (heat-production $= KW^{2/3}$), in which the importance of the surface, $W^{2/3}$, is gradually removed with the increasing elimination of the activity of that special nervous mechanism that regulates heat-production when heat-loss is the dominant circumstance. But, if, on the other hand, attention must be paid to it—and why not?—then some consideration must be given to some influence of the weight or of the mass of the body other than as a value which determines the extent of its surface. This may conceivably be dealt with under two headings.

Thus, on the one hand, it may be thought that the weight, *per se*, makes

its appearance in these expressions because the weight of the limbs that are moved is at first a large fraction of the whole sum of mechanical work which is performed. Or, on the other hand, it might be considered that W enters in some less direct fashion as a representative of mass rather than of weight, since it is clear that the value "heat-production/mass" must to some degree determine the temperature of the musculature, and further, as is well known to be true, the heat-production would be expected to vary with this temperature, as well as with the mechanical work which is performed.

On either of these assumptions as to the importance of W, it is conceivable that at first the expression for the heat-production of rest was complicated in such a fashion that

$$\text{Production/Surface} \quad \text{became} \quad \text{Production/(W} \times \text{Surface)};$$

that is to say, that the denominator might change from $W^{2/3}$ to $W^{5/3}$. Now we have only to consider that the withdrawal of the dominating influence of the surface, the removal of one kind of automatic nervous control, passes on further than to mere elimination until the surface becomes now the humble agent of the heat-production under the influence of a reversed nervous control. This is, indeed, what actually happens, but is it not the case, this being so, that now the heat-production will tend to vary inversely as the surface?

$$\frac{\text{Heat-production}}{\text{W} \times \text{Surface}} \quad \text{now becomes} \quad \frac{\text{Heat-production} \times \text{Surface}}{\text{W}},$$

and the denominator changes from $W^{5/3}$ to $W^{1/3}$.

Now, finally, we come to consider the state of affairs represented by Group D, where, apparently, both weight and surface have dropped out of account. Here it is open to us to consider that the surface has now become a cubic quantity with the same dimensions as the weight, and that they are thus both cut out from the expression for the heat-production. Nor is this so absurd as might at first appear, since the process of sweating introduces a further quantity, by which the extent of the surface is multiplied, and might very well be regarded as a third dimension. Or else we revert to the position that the weight of the limbs becomes less and less important as the amount of externally useful mechanical work rises in value, and that in this way there is a tendency to minimise the value of W in the denominator. If this is so, then it is to be anticipated that a further continuance of these experiments will lead to the further observation that, at a still higher level of work-performance, the heat-production will be found equal to KW^{-n}, and the weight then have an appearance of being a positive advantage, since there is then no obvious reason why the surface should disappear from the numerator.